21 No. 3 (New Series No. 81) January 1961

ANALYSIS

Edited by
BERNARD MAYO
with the advice of

A. J. Ayer
R. B. Braithwaite
Herbert Dingle
A. E. Duncan-Jones
P. T. Geach
C. A. Mace
A. M. MacIver
H. H. Price

THE UNIVERSITY OF MICHIGAN
JAN 27 1961
PERIODICAL READING ROOM

CONTENTS

Telepathy
I. THALBERG

On Substitutivity Criteria
DONALD J. HILLMAN

A Myth about Logical Necessity
JONATHAN BENNETT

Ryle on Namely-Riders
P. T. GEACH

The Hardness of the Logical 'Must'
EDWARD J. NELL

THREE SHILLINGS AND SIXPENCE NET

BASIL BLACKWELL · BROAD STREET · OXFORD

AN

W
pers
ache
Prof
as J
know
such
cons
qual
sion
meta
(ibid
that
chok
I
has
whe
minc
Tom
origi
V
is m
with
Mrs.
of P
phen
very
V
exig
cons
to ar
exch
of lo
phen
not
whic
parti

TELEPATHY

By I. THALBERG

WRITERS on the problem of other selves frequently ask whether telepathy might enable us to scale the battlements of another person's mind, and become directly acquainted with the sensations, the aches, twinges, itches, terrors and rages which he shelters therein. Professor John Wisdom, for instance, examines a number of cases, such as Jones receiving images in a blue frame or with the label 'Smith' and knowing 'that such and such an image signifies that Smith is seeing such and such a thing' (*Other Minds*, Oxford, 1956, p. 95). After due consideration, Wisdom decides that telepathic experiences will not qualify as acquaintance with another person's feelings or sense-impressions. He declares: 'telepathy's not . . . what is wanted by the real metaphysician when he asks for real knowledge of the mind of another' (ibid., p. 98). It is patent, observes Wisdom, that when Arabella judges that Bill is angry, not from attending to Bill's demeanour, but from the choking in her own throat, she is still acquainted only with *her* experience.

In this brief note, I will simply ask what it means to claim that Tom has telepathic acquaintance with Bill's feelings; I will *not* argue (1) whether telepathic experience constitutes real knowledge of another mind; or (2) whether there is any sense in talking of the *identity* of Tom's nausea and Bill's *Abbild* of Tom's nausea, i.e., whether the original and the telepathized feeling can possibly be compared.

We should observe, initially, that the traditional notion of telepathy is much too elastic to satisfy a philosopher's criterion of 'acquaintance with another individual's mental states'. Even a sophisticated writer like Mrs. Sidgwick, in her article 'Telepathy' (in J. M. Baldwin's *Dictionary of Philosophy*, London: 1901–5), extends the notion to comprise such phenomena as one person receiving a vision of a distant friend at the very moment when the friend is dying.

Why won't the popular view of telepathy meet the philosopher's exigencies? I take it that the profane conception is that telepathy consists, for the most part, in the transference of *thoughts* from one mind to another without the benefit of overt methods of communication, e.g., exchange of verbal signs, gestures, smoke-signals. The pattern is a kind of long-distance conversation *sans* telephone or wireless. The philosopher cannot accept this view of telepathy for the simple reason that it does not imply that the telepathic observer is acquainted with something which is ordinarily inaccessible to him. For one can ask, 'Is there any particular item in a man's stream of consciousness when he is thinking,

as there is when he is smelling an acrid odour, feeling a throb of pain, or fuming with anger?' It is not clear that there is such an item of inner experience when we have a thought: surely whispered words *needn't* accompany thinking; and furthermore one can mutter a sentence and still deny that one was thinking about what it means, as a broker might recite 'Church Monuments' while studying the Dow-Jones averages. No: the philosopher is interested in becoming acquainted with states of mind of which at least one characteristic element is privy to the person who is in that state of mind; and sensations and emotions are more evidently of this type than thoughts.

But suppose, nevertheless, that to communicate a thought, to make a listener understand your words, it is necessary to produce a feeling in the listener's mind: in this case, the popular model for telepathy—speaking over the telephone, or even a *tête-à-tête*—will not do. To appeal to this model begs the question: for it is not obvious that when we listen to another person, and understand what he says, that our understanding is accompanied by any inner feelings. Surely an electrician's apprentice can follow the instructions of the master electrician, repeat or paraphrase them for another apprentice, and apply the instructions to similar cases, i.e., understand the instructions—and yet the apprentice can deny having any particular inner feelings. Perhaps this kind of understanding should not be called 'communicating a thought'; but if not, what kinds of occurrences do we have in mind? Whatever we mean by 'thought-transference', a conversation—telephonic or face-to-face—will not qualify both as thought-transference *and* acquaintance with another person's feelings; the reason is that a conversation need not involve having *any* feelings at all, much less your interlocutor's feelings.

Therefore let us neglect vulgar notions of telepathy, and pass on to mental states for which there is a fairly definite range of associated inner sensations. A case of this sort would be the way Tom feels when he is suffering from a head cold. Bill can see Tom's inflamed nose, his rheumy, bloodshot eyes, his grimaces; he can hear Tom's sneezes and expectorations, and feel the heat of his brow. Yet Tom has some sensations which Bill does not have: tickling in his nose, dryness of his throat, smarting eyes, a cottony feeling in his cerebrum, and general lethargy. Therefore when Bill says, consolingly, 'I know just how you feel', he cannot possibly mean: 'I am acquainted with your feelings'. Regardless of how many colds Bill has endured, no matter how minutely he has noted what feelings he had when he displayed the same symptoms as Tom, Bill cannot ever be sure that these symptoms of Tom's are accompanied by feelings similar to his own.

A number of telepathic remedies have been suggested to supply the *lacunae* in Bill's observations. Remembering that we are not concerned to ask whether these remedies would vindicate Bill's claim to know

how Tom feels, consider how telepathy might be said to acquaint Bill with Tom's discomfort. Bill might claim acquaintance with Tom's feelings in at least two interesting senses of 'feeling'; (1) The short-range sense, in which a particular throb of pain or a tickle would constitute the objects of acquaintance; (2) The long-range sense, in which the object would be an enduring feeling, a series of feelings, or one changing feeling. To illustrate: (1) 'I know the sharp pain you felt when you barked your shin on the chair'; (2) 'I know the terror you felt as the police-dog chased you around the block', or 'I know the exaltation and excitement you must have had during your climb up the Matterhorn', or 'I know how your discomfort must have increased to the point of agony when they set your leg without anaesthetic'.

Consider short-range feelings. I have already mentioned Wisdom's examples of one person having a feeling, and knowing that another person is also having that feeling. Is this what is needed for telepathic acquaintance? For example, if Bill felt a tickle when Tom wrinkled his nose, would we say that Bill is acquainted with one of the feelings of Tom's cold? I would argue against such a conclusion. If Bill feels no other discomforts; if, unlike Tom, who is struggling with his cold, Bill feels quite fit, except that he is afflicted with a tickle in his nose; then I would say that Bill is not acquainted with Tom's tickle, because there is a crucial difference between Tom's tickle and Bill's tickle. The difference can be brought out by the disparity in their reactions. While Tom might groan, 'Lord, here goes another sneezing fit!' Bill might be rather surprised to have a tickle in his nose when he is generally comfortable. In other words, the tickles are interpreted quite differently by the two men, just as a soldier and a general who are watching the same battle might see, respectively, a confused mêlée and a pincer-movement.

The same objections could be made against the use of Wisdom's 'mental state detector', a machine which, when trained on Tom's head, gave Bill a tickling sensation when Tom felt a tickle, and so on. If we continue to restrict ourselves to the short-range sense of 'feeling', then we must agree that when Bill calmly regards Tom through the mental-state detector, and feels irritation in his nose, this is quite different from the distressed reaction of Tom at the sign of more suffering.

It is even more obvious that two people cannot be said to feel the same during an extended period of time, e.g., feel the same anguish while waiting for news about a friend who is lost on a mountain-climbing expedition, unless the feeling occurs in a similar mental context. 'After all,' one person might say, 'you've only known him two years; I've climbed with him since adolescence; and if I had been along, he might not have got lost.' Or, to vary the illustration, it would be odd for a man to claim that he was acquainted, telepathically, with the feelings of a

friend who is swimming the English Channel, but admit that he nevertheless felt dry and warmly clothed. Feeling so comfortable, how could he claim to share his friend's misery?

To return to the case where Bill claims acquaintance with Tom's feelings during a week-long bout of flu: it is evident that telepathic transference of Tom's inner feelings of congestion, shortness of breath, etc., would not constitute 'feeling the way Tom did'. For we must suppose that Bill's experiences were appropriate to his own activities during the week of Tom's illness: Bill went to the office every day, dressed in a business suit, not pyjamas; he attended films or parties in the evening; his eyes appeared clear, not bloodshot and watery; his cheeks were rosy, not flushed, and so on. We needn't consult the mental-state detector again to establish the conclusion that mere transference of a few symptomatic inner sensations does not constitute acquaintance with another person's feelings during a given period of time. In the imagined case, Bill does not have the same feelings as Tom, because Tom feels as if he had a cold, while Bill, except for an inexplicable feeling of congestion, and so forth, does not feel as if he had a cold, for he is able to go about his normal life.

These reflections suggest that telepathic transference must include more of the context of the feelings of a cold, for Bill to be acquainted with Tom's cold. Now suppose that Bill noticed streaming eyes whenever he looked at himself in the mirror; that, besides the tickle in his nose, he suffered from paroxysms of sneezing; that he felt too weak to continue working. In short, when Bill is in the same frame of mind as Tom, would we say that Bill is acquainted with Tom's cold-feelings?

If we require that telepathy include *all* Tom's sensations, thoughts, abilities and memories, i.e., if Bill must feel like Tom in every respect to claim acquaintance with Tom's cold, then there would be no Bill to receive the replica of Tom's mental state. Bill would see Tom's face when he looked in the mirror, and observe Tom's coated tongue when he opened his mouth. Bill would not remember, longingly, his own days of robust health, but he would recall Tom's fishing-trips and mountain-climbing expeditions.

The suggestion that Bill must be in the same frame of mind as Tom in *every* respect, to have telepathic acquaintance with Tom's feelings of a cold, is too fanciful. So shall we say that telepathic transference comprises only Tom's feelings of discomfort and those aspects of his mental outlook which are relevant to his having a cold? Assume that Bill trains the mental-state detector on Tom's cranium, and feels all the cold-feelings which Tom feels. In these circumstances, we would not hesitate to say that Bill is acquainted with Tom's cold; there would be no hypocrisy in Bill's compassion for Tom. The only difficulty is that we would have to admit that Bill, as well as Tom, *has* a cold.

If real telepathic acquaintance with another person's cold requires that one feel as if one had a cold oneself, why call this telepathy? We must assume that Bill's telepathic acquaintance cannot be distinguished from feeling ill himself. If Tom felt ill but Bill did not, we could object that Bill did not really feel like Tom. But then are there any grounds for claiming that only Tom is really ill, but Bill only receives Tom's feelings of illness, without being ill himself? In short, genuine telepathy would be possible only if Tom and Bill felt *and* behaved alike, in all the respects that are relevant to having a cold. Since Bill can observe his behaviour, or at least be told about his behaviour by other observers, any behavioural clue that Bill *is not* ill could make Bill feel less ill. For example, if a physician pointed out that Bill's respiration is perfectly normal, Bill might begin to feel more comfortable.

Therefore if Bill felt and looked like Tom in all relevant respects, there would be no reason to deliver a different medical diagnosis of Tom and Bill. For practical purposes, they would both have a cold. If this case is typical of telepathy, then to claim that there are such occurrences is merely to assert that two people can feel exactly alike in some respect. However, I fail to understand the further claim, ' but only Tom really *has* a cold; Bill only feels Tom's feelings of a cold '. What evidence is there for saying that only Tom is ill? What lack in Bill's feelings and conduct makes us say that he is only receiving a telepathic replica of Tom's feelings? ' Being acquainted, telepathically, with another's feelings ', if my analysis is correct, is nothing less than ' Being in the same condition as the other person '.

Oberlin College

ON SUBSTITUTIVITY CRITERIA

By DONALD J. HILLMAN

THE idea of substitutivity is common to many philosophical arguments. The particular sort of substitutivity I want to talk about, however, is concerned with mutual replaceability of linguistic forms. Following Leibniz, we talk of interchangeability *salva veritate*: *Eadem sunt quorum unum in alterius locum substitui potest, salva veritate.* More recently, we read that 'we can describe any two linguistic forms as cognitively synonymous when the two forms are interchangeable . . . *salva* . . . *analyticitate*'.[1] In a different key, and with different terminology, we have 'Two proposition-factors are of different categories or types, if there are sentence-frames such that when the expressions for these factors are imported as alternative complements to the same gap-signs, the resultant sentences are significant in the one case and absurd in the other'.[2] It is clear that here the expression 'alternative complements to the same gap-signs' refers to interchangeability.

If we are prepared to overlook the fact that the last rule is not explicitly concerned with the preservation of anything, it is convenient to regard each of these rules as instances of a general principle based upon substitutivity *salva aliqua qualitate*, to coin a somewhat pedantic phrase. It is important to remember that we are here discussing substitutivity and not substitution. We must carefully distinguish between the logical problem of providing a correct formulation of the rule of substitution and the philosophical problem concerning substitutivity *salva aliqua qualitate*. Only the latter question will be discussed here. But just as it is necessary to make the above distinction, so it is necessary to distinguish clear areas of applicability of the various substitutivity criteria we are concerned with. I want to emphasize the importance of relating criteria to their domains of applicability and to suggest that arguments that ignore boundaries invariably end in stalemate. To illustrate my main point, I should like to discuss an argument which transgresses the boundary separating a colloquial language from a formalized language, thus bringing about a complete standstill.

In the course of an investigation of Frege's distinction between function and concept, Professor Max Black has written the following:

> Frege's view that a function sign is an incomplete designation is complicated by his view that declarative sentences are designations—either of 'the True' or of 'the False'. . . . The following argument seems to me to be a sufficient refutation of Frege's view that sentences are designations of truth values.

[1] W. V. Quine, *From A Logical Point of View*, Harvard, 1953, p. 32.
[2] G. Ryle, 'Categories', *Logic and Language*, 2nd Series, Ed. Flew, pp. 77–78.

> We may assume that if A and B are designations of the same thing the substitution of one for the other in any declarative sentence will never result in nonsense. This assumption would not have been questioned by Frege. Let A be the sentence ' Three is a prime ' and B the expression 'the True'. Now 'If three is a prime then three has no factors' is a sensible declarative sentence; substitute B for A and we get the nonsense ' If the True then three has no factors '. . . . Hence, according to our assumption, A and B are not designations of the same thing—which is what we set out to prove.[1]

Clearly *two* assumptions have been made in this passage. The first has been explicitly formulated as a substitutivity criterion, viz.:

(1) If A and B are designations of the same thing, the substitution of one for the other in any declarative sentence will never result in nonsense.

The second assumption, not mentioned by Black, corresponds to the adoption of a semantical rule, which might be described as:

(2) In any non-oblique context, the expression ' the True ' designates the True.

Professor Church has criticized as fallacious the argument I have selected.[2] He argues that the rebuttal of Frege's claim that sentences are designations of truth values by appeal to the grammar of conventional English is not legitimate. We should not be concerned with transgressions of the rules of any natural language; on the contrary, ' the right question to ask here is not what, by existing custom, are the rules of English, but rather what it is desirable to take as the rules of a formalized language. In a suitable formalized language the analogue of " If the True then three has no factors " does have a use, namely as a designation of the False '.[3]

A number of important things come out of this criticism. In the first place, Church is entirely correct in pointing out that Black's argument concerns a technical aspect of Frege's work that can only be investigated in technical ways, in this case within the framework of a formalized language. This comment, moreover, is particularly true of assumption (2). Frege himself argued the necessity for suitable formalized language systems in *Grundgesetze der Arithmetik* and the earlier *Begriffsschrift*. In any such system, the analogue of ' the True ' will designate an object, and it is *only* by reference to an antecedently contrived formalized language with unambiguous rules of designation that the admissibility of assumption (2) can be settled. A second important consequence of Church's criticism is that when Black's argument is transferred *in toto* to the realm of formalized language, the substitutivity criterion (1) becomes inapplicable. What we need in this case is a

[1] ' Frege on Functions ', *Problems of Analysis*, p. 235.
[2] Review of ' Frege on Functions ', *Journal of Symbolic Logic*, Vol. 21, No. 2, June 1956, pp. 201–202.
[3] *ibid.*

criterion to the effect that formalized languages containing propositional calculus allow the introduction of primitive constants denoting truth values and permit their substitution for the propositional variables.[1] The difference, moreover, between the notions of substitutivity involved in this criterion and criterion (1) shows that Professor Black's assumption does not hold for his argument. His mistake was to misjudge the domain of applicability of his substitutivity criterion.

I think it is now clear that (1) cannot be used to support the argument in question. However, the possibility remains that (1) *can* be used to support arguments of a different nature. The unsuitability of (1) with respect to Black's argument is due to the inappropriateness of invoking a non-technical criterion to deal with a technical question of Frege's theory of designation. Nevertheless, it seems possible to extract (1) from its present context and to test its use in questions of informal analysis. For there is certainly something very general about this assumption, in that it appears to uphold an intuitive conviction about extensionally identical expressions of a natural language. It has, for example, a conceivable use in supporting arguments concerning the co-extensiveness of proper names and definite descriptions, or of general terms and their essential or accidental predicates. Let me therefore test the use of (1) in the field of informal analysis, i.e. its correctness when applied to sentences of a natural language, such as English.

One counterintuitive consequence of (1) comes immediately to mind. Consider, for example, the declarative sentence 'All Americans who enlisted under Julius Caesar were active Republicans'. It is immaterial whether this sentence is true or false, or whether it may, or may not, be used to achieve reference. We need only recognize that it is a 'sensible', i.e. well-formed, declarative sentence of English. In this sentence, the expression 'All Americans who enlisted under Julius Caesar' denotes nothing. Similarly, the expressions 'a griffin', 'the largest prime', 'the sixth finger on President Eisenhower's left hand', etc., are all designations of nothing. Thus all the expressions so far listed have the same reference, that is, they all designate the same thing. Yet none of the last three can be substituted for the first in the declarative sentence 'All Americans who enlisted under Julius Caesar were active Republicans' without getting nonsensical consequences. The situation is no better when we come to consider declarative sentences that *do* have a referring use. For example, we cannot replace the expression 'twelve' by the expression 'the number of the Apostles' in the sentence 'There are twelve numerals on the face of Big Ben' without reducing the sentence to nonsense, although the expressions in question surely designate the same thing. Examples such as these, and there are many of them, show

[1] See, for example, Alonzo Church, *Introduction to Mathematical Logic*, Vol. 1, §10, pp. 69–74.

that (1) is not a well-founded assumption, since it often rules out the possibility of two different syntactic constructions standing for the same thing, which is a possibility we want to allow. We are thus no better off in trying to establish the applicability of (1) to sentences of colloquial English than was Professor Black in his attempt to apply it in his refutation of Frege's claim that sentences are designations of truth values.

The original failure of (1) was ascribed to a misjudgment as to the domain of its applicability. Now we have detected another failure, and the question naturally arises whether this is ascribable to the same cause. I shall argue below that the failure of (1) as a substitutivity criterion governing the expressions of a natural language stems not so much from a boundary misjudgment as from a boundary neglect. I mean by this that (1) fails as a criterion for English sentences not because it cannot be applied in this connection, as in the case of Professor Black's argument, but because it cuts across the boundary separating colloquial English from a formalized language, and combines elements from each territory in an illegitimate way.

To substantiate this claim, I argue that (1) is really a conflation of two quite distinct substitutivity criteria, which we may phrase as follows:

(1a) If A and B are designations of the same thing, the substitution of one for the other in any declarative sentence will not result in a change of truth value for that sentence.

(1b) If A and B are designations of the same type, the substitution of one for the other in any declarative sentence will never result in nonsense.

If we agree that (1a) and (1b) together afford a reasonable reconstruction of Black's assumption (1), the reason for its failure as a substitutivity criterion for colloquial English sentences becomes quite clear. The practicability of (1a) hinges on our ability to assign truth values in a systematic way, and this is possible only for formalized languages. (1b), however, is a venerable assumption of ordinary language. Thus (1) fails to hold for the sentences of *colloquial English* because it neglects the boundaries of applicability of its components. It collapses because it borrows freely from two criteria whose domains, in this connection, are distinct, and attempts to combine its borrowings in an illicit manner.

Even so, the question might still be asked whether (1) can ever be regarded as the result of combining (1a) and (1b). (1b) might also be applied to the well-formed sentences of a formalized language, so that any subsequent merger of (1a) and (1b) appears to avoid the danger of a boundary trespass. Let us examine this question further. The sort of combination we ruled out as illicit above is the *miscibility* of (1a) and (1b) to form a substitutivity criterion for the sentences of colloquial English. Could we, however, *derive* (1) from (1a) and (1b), and then use it as a

criterion governing the intersubstitutivity of expressions in a formalized language? I think this question must be answered in the negative. (1a) and (1b) do permit the derivation of a compound criterion, but all it amounts to is the following:

I. If A and B are designations of the same thing and are of the same type, then if one is substituted for the other in any declarative sentence, the result will never be nonsense.

I is not, of course, equivalent to the conjunction of (1a) and (1b), but is certainly derivable from them. (1) however, is not derivable from I, so that (1a) and (1b) do not yield (1).

Let me summarize the main points of my argument so far. Professor Black's assumption (1) cannot be used to support his claim that sentences are not designations of truth values. His mistake was to misjudge (1)'s domain of applicability. Nor does (1) hold as a substitutivity criterion for colloquial English sentences. The failure of (1) in this case stems from a boundary neglect, if, and it is surely justifiable to do so, (1) is considered to be a mixture of (1a) and (1b). Finally (1), as a criterion for formalized languages, is not derivable from the assumptions that constitute its underpinnings. Thus, on all counts, (1) has to be rejected as a genuine substitutivity criterion.

I should like to add one final word of caution before bringing this paper to a close. It should be made quite clear in any discussion concerning the interchangeability of linguistic forms that the ' nonsense ' to which Professor Black refers stands for non-well-formedness or ungrammaticalness. Otherwise, we run the risk of identifying this term with absurdity, which results, in Professor Ryle's opinion, from ' the improper coupling not of expressions but of what the expressions signify '.[1] The diagnosis of category-mistakes, as in, for example, ' Saturday is in bed ',[2] is a wholly different procedure from discriminating category or type dissimilarity, in the syntactic sense. A category-mistake is to be looked for in syntactically regular sentences that are nevertheless absurd in that certain of their constituent parts are not of the right type to be combined with the remaining constituent parts. The successful use of the substitutivity criteria discussed in this paper, however, depends mainly on our ability to distinguish between well-formed and non-well-formed English sentences. Constructive attempts to supply an algorithm accomplishing this aim are to be found in the work of Ajdukiewicz[3] and Lambek.[4]

Lehigh University

[1] ' Categories ', *Logic and Language*, 2nd Series, p. 77.
[2] *ibid.*, p. 75.
[3] ' Die Syntaktische Konnexität ', *Studia Philosophica*, Vol. 1, 1935, pp. 1–27.
[4] ' The Mathematics of Sentence Structure ', *American Mathematical Monthly*, Vol. LXV, No. 3, March 1958, pp. 154–170.

A MYTH ABOUT LOGICAL NECESSITY

By JONATHAN BENNETT

IN these few pages I shall try to demonstrate the emptiness of the most cumbersome piece of unexamined intellectual baggage at present being hauled about by English philosophers. I here cite one example to be going on with, at the end of the paper I shall give a handful more, and it would be easy to multiply the number by ten from the writings of reputable philosophers.

> The outstanding philosophical achievement of the half-century which has just drawn to a close [*i.e.* the period 1900–1950] has been an appreciation of the peculiar status of *a priori* judgments and of logically necessary or formally true propositions. . . . Though many problems remain unsolved, the main outline is now clear: formally true statements assert nothing about the world; instead, their function is to state principles according to which empirical propositions are deduced from other empirical propositions. . . . (R. B. Braithwaite, 'Moral Principles and Inductive Policies', *Proc. Brit. Acad.* 1950).

What is wrong with this passage and with the myth of which it is an expression is its assumption that we have clear notions of what it is for a proposition to be logically necessary and of what it is for a proposition to assert something about the world, these notions being such that it is plausible to say that it has recently been discovered that every proposition having the first of these properties lacks the second. This assumption is wrong: there is no body of published theory giving a clear account of such notions, and despite fairly diligent searching I have so far failed to find, among the many philosophers who accept the myth, one who is able when challenged to supplement the literature on this vital point.

Let me make it clear at once that I am not going to defend synthetic *a priori* truths—I am going to attack a popular mishandling of the truth that all necessary truths are analytic, and through this attack to draw right-wing conclusions from left-wing premisses. For the myth which I am trying to nail does have its origin in a view, which I believe to be a correct view, about the *means* whereby logical truths must be established. I shall state this view in terms of necessary conditions for the deductive validity of an argument rather than necessary conditions for the logical truth of a proposition: for all present purposes the two are interchangeable. The view goes like this: If one wishes to defend the claim that Q follows deductively from P, one possible course of action is to produce a proof of Q from P, that is to produce a series of statements $R_1, \ldots R_n$ such that R_1 follows from P, each R_i follows from the preceeding ones, and Q follows from one or more of the R_is. Now if there is also a

challenge to one of *these* claims about following-from, it may again be possible to produce a proof in justification. But it will not always be possible to meet a challenge in this way, for a stage will eventually be reached at which the defender of the proof will wish to say that the steps in the proof are direct or immediate in such a way that he cannot insert any more intermediate lemmas. To say of a given R_j and R_k that the inference from the former to the latter is too immediate to allow of proof by the interposition of lemmas is to say that one is defining R_j as meaning what R_k means (and perhaps meaning more than this as well); or it may be to say that R_j does in general mean what R_k means—that, for instance, if anyone were asked what R_j means it would be generally considered natural and helpful for him to produce R_k by way of explanation or partial explanation. In the sequel, I shall use the expression 'follows immediately from' and its cognates for this relationship which can be defended not by proof but only by stipulation or by description of linguistic activity. The view I am here concerned with, then, is the view that, although Q may follow from P without following immediately from P, it cannot follow from P unless there is a chain of propositions joining P to Q each link in which is an immediate sequence. This may be called the conventionalist view of logical necessity (or of deductive validity), or the view that all necessary truths are analytic and all deductively valid arguments are in a certain sense analytically valid.

This is a theory about the bricks out of which logical truths must be built, and I accept it as true, subject only to some Quinean reservations —which are not here relevant—about its naive employment of intensional terminology. But far too often this theory is confused with a quite different theory about the sort of structure which can be made with that sort of brick: in particular, it is often said that *at last we understand* that out of such materials nothing can be constructed which is informative about the world. This is the parrot-cry to which I should like to put a stop.

It is not difficult to see how it has arisen. If Q follows immediately from P, then there is a plain sense in which it is true that Q tells us nothing not told us by P, and in a parallel way true that $P \supset Q$ tells us nothing at all. For in such a case, a person who understands both P and Q cannot know that P and wonder whether Q, he cannot feel enriched by the knowledge that $P \supset Q$, he cannot regard himself as having been carried forward from P to Q, he cannot understand someone who says that he accepts P and rejects Q; and so on. Now it is tempting to move from saying this to saying that if Q follows from P at all, even if not immediately, then Q must be in the same way non-novel with respect to P—for we have agreed that the relation of consequence is the ancestral of the relation of immediate consequence. But to argue in this way is to commit a howler, just as it would be to argue that since parents always

resemble their offspring and the relation ' is an ancestor of ' is the ancestral of the relation ' is a parent of ' therefore ancestors always resemble their descendants. The howler there is that resemblance is not transitive, and the howler in the other case is that the relation ' is non-novel with respect to ' is, in the sense in which I have expounded it, not transitive either. If we are to base any claim about what sorts of statements are logically necessary on the view that all necessary statements are analytic in the sense here expounded, we shall have to find some *transitive* relation which always accompanies the relation of immediate consequence. (Compare: the substitution of ' is born earlier than ' for ' resembles ' in the argument about ancestors.) There is one such relation: philosophers have known about it since Hume and, if they had paid attention, they could have known about it since Leibniz. What Hume re-discovered was that if Q is an immediate consequence of P then there cannot be a time-reference in Q later than the latest time-reference in P; this relation of not-being-dated-later-than is transitive, and so it accompanies not just every immediate consequence but every consequence. It is obvious that this discovery has important consequences for causal rationalism. It was a splendid discovery, and so far as I know it is the only discovery which has ever been made to the effect that a certain limit obtains on the *results* which can be achieved by purely deductive means.

And yet it is said, over and over again, that we now have in our hands a weapon which will cut down not only the causal rationalist who thinks that logic could in principle tell him what will happen next, but also every manner of rationalist including those who, like Leibniz, hold no brief at all for causal rationalism but who are in a non-modern way optimistic about the possibilities of discovery by deductive means.

There is a standard reply to the foregoing line of argument. With monotonous regularity one is told that this argument shows only that from a given premiss one may validly deduce a conclusion which is psychologically new, but the big discovery is that by purely deductive means we cannot arrive at conclusions which are *really new*, which are *factually new*, whose *empirical content goes beyond* that of the premiss, which are *not just a restatement* of what has already been (implicitly) said in the premiss . . . etc. Now, what does all this mean? So far as I have been able to discover, the remark that the conclusion does not really go beyond the premiss means only that once the premiss is known there is no need for further empirical investigation in order for one to know the conclusion. But this is just a trivially immediate consequence of the hypothesis of the whole discussion, namely that the conclusion follows deductively from the premiss. Again, it is sometimes said that the conclusion goes no further than the premiss in the sense that every state of affairs excluded by the conclusion is already excluded by the premiss;

but, again, to say this is to say only that it is logically impossible that the premiss should be true and the conclusion false, which is another trivial consequence of the hypothesis of the discussion.

I am not objecting to the fact that the alleged discovery about logical truths turns out to be logically true. I am objecting to it on two counts. Firstly, it is normally presented as a consequence of the discovery that logical truths are analytic, whereas in fact it has nothing to do with that at all. This is no sooner said than seen, and I shall not comment further upon it here. Secondly, the 'discovery' turns out to be not just logically true but the sort of logical truth which no sensible person could possibly make a mistake about. For is it even slightly plausible to suggest that any moderately competent philosopher has ever said, or shown that he believed, that there could be a P and a Q such that Q was validly deducible from P and also such that after P was known further empirical investigation was needed before Q could be known? Or: such that Q followed deductively from P and also such that a logically possible situation would instantiate both P and a contrary of Q?

'But what *are* we to say about necessary truths?' I have been plaintively asked. The answer to this is: say what you like about them, just so long as what you say is true. Say, for example, that claims about logical necessity and deductive validity are arrived at and defended in ways which entitle us to say that if 'analytic' has any legitimate use then all logically necessary truths are analytic. Say also, if you wish, that the meaning-claims which lie at the back of this are based upon usage rather than upon any inspection of 'ideas' in the manner of, notoriously, Descartes and Locke. Say that Hume was right about causal rationalism, and that his rightness follows from the discovery that all logical truths are analytic.

If you like, say also that if Q is deducible from P then it is the case that Q is implicit in P, and that no possible situation excluded by Q is allowed by P, and that knowing P to be true completes all the empirical investigation needed for one to know Q to be true, and so on. Say all this, but do not slip into thinking that any of it depends upon the thesis that all logical truths are analytic—for in fact you have only recited some trivial consequences of the holding of a deducibility relation, on *any* theory about deducibility or logical necessity. Do not think, either, that you are saying anything interesting: no-one has ever denied that if Q is deducible from P then these other relations also hold between P and Q.

'What are you going to do about the deductive metaphysician?' I have also been asked. Well, I am not going to do anything about him: I shall continue to study some metaphysicians and not others, to admire some and not others, and no doubt sometimes to find that a piece of metaphysical reasoning is invalidated precisely by inattention to general questions about the nature of logical truth. But there is no onus on

anyone to provide new weapons with which to thump the deductive metaphysician as such—there never were any such weapons, only the illusion that they existed.

A few more examples:

> Analytic propositions . . . are entirely devoid of factual content. . . . (A. J. Ayer, *Language, Truth and Logic*, p. 79 of 2nd edition.)

> The expression 'Every event has a cause' is—owing to the impossibility of describing any circumstance that could show it to be false—vacuous and utterly uninformative . . . (G. J. Warnock, '"Every Event Has a Cause",' *Essays in Logic and Language*, 2nd series, p. 109.)

> A corrigible proposition gives you some information about the world—a completely incorrigible proposition tells you nothing. (Douglas Gasking, 'Mathematics and the World', *Essays in Logic and Language*, 2nd series, p. 208.)

> Few people now think, as Descartes seems to have done, that we can arrive at scientific conclusions about matters of empirical fact . . . by deductive reasoning from self-evident first principles. The work of Wittgenstein and others has to a great extent made clear the reasons for the impossibility of doing this. It has been argued, in my opinion convincingly, that all deductive inference is analytic in character; that is to say, that the function of a deductive inference is not to get from the premisses 'something further' not implicit in them . . . but to make explicit what was implicit in the conjunction of the premisses. (R. M. Hare, *The Language of Morals*, p. 32.)

> Modern logicans, for the most part, regard pure mathematics as analytic, but consider all knowledge of matters of fact to be synthetic. (Bertrand Russell, *Philosophy of Leibniz*, preface to 2nd edition.)

University of Cambridge

RYLE ON NAMELY-RIDERS

By P. T. GEACH

> 'I proceed. "Edwin and Morcar, the earls of Mercia and Northumbria, declared for him: and even Stigand, the patriotic archbishop of Canterbury, found it advisable—"'
>
> 'Found *what*?" said the Duck.
>
> 'Found *it*' the Mouse replied rather crossly: 'of course you know what "it" means.'
>
> 'I know what "it" means well enough, when *I* find a thing', said the Duck: 'it's generally a frog or a worm. The question is, what did the archbishop find?'
>
> *Alice in Wonderland*, c.ii.

I SHALL here critically examine the doctrine of reference put forward by Professor Ryle in his article 'Heterologicality'.[1] I shall try to show that this doctrine is inadequate, and that the alleged proof that we cannot rightly ask whether 'heterological' itself is heterological is fallacious. I shall not in the least be concerned with Grelling's paradox itself, and my remarks have no bearing on its solution. '—is heterological' means '— (is an epithet that) lacks the property which it stands for'; my discussion will relate not to the terms 'property' and 'stands for', whose use would have to be examined if I were concerned with Grelling's paradox as such, but to the logical role of the pronoun 'it'.

The crucial passage of Ryle's discussion[2] is the following one. (For the sake of clarity, I have taken the liberty of slightly altering the format of the passage.)

> 'If unpacked, the assertion that "heterological" is heterological would run:
>
> "heterological" lacks the property for which it stands,
> namely, that of lacking the property for which it stands,
> namely, that of lacking. . . .
>
> No property is ever mentioned, so the seeming reference to such a property is spurious.'

The procedure of unpacking that Ryle employs goes on the principle that a "referring expression" calls for a "namely-rider"; and in applying the principle he assumes that the phrase 'the property for which it stands', at both its occurrences in the unpacking, is a "referring expression". Now as regards the first line of Ryle's unpacking:

(1) 'heterological' lacks the property for which it stands

[1] ANALYSIS, 11.3, 1951: reprinted in *Philosophy and Analysis*, Blackwell, 1954.
[2] P. 51, in the reprint of the article.

the demand for a namely-rider seems natural and legitimate: namely, which property? For which property does 'heterological' stand?

Let us, however, go more slowly. It may be conceded that in the proposition:

(2) 'heterological' lacks the property for which 'heterological' stands

the last six words are an expression that calls for a namely-rider. And the pronoun 'it' in (1) may well seem to be merely, as we may say, a *pronoun of laziness*—a grammatical dodge to avoid inelegant repetition of words. If this were correct, the last six words of (1), no less than those of (2), would be an expression that called for a namely-rider.

It is questionable, though, whether 'it' in (1) is a mere pronoun of laziness. We may see this if we contrast this 'it' with what is indubitably a mere pronoun of laziness: the pronoun 'that', at both its occurrences in Ryle's unpacking, is a mere elegant variation, to avoid repeating the words 'the property'. (For logical purposes, repetition is more perspicuous than elegant variation; so in the sequel I shall speak as though Ryle's unpacking had actually contained 'the property' instead of these occurrences of 'that'). But 'it' in (1) is not mere elegant variation. For if we consider the predicates in (1) and (2), namely:

(3) — lacks the property for which it stands
and (4) — lacks the property for which 'heterological' stands

respectively, it is clear that (3) and (4) are not at all the same. (Indeed, on the face of it, since (3) is an expansion of 'is heterological', (3) is true of just those epithets of which (4) is *not* true; that is precisely what gives rise to Grelling's paradox.)

Two things may hinder us from seeing this point. First, it seems clear that (1) says the same as (2)—if either sentence can be successfully used to say anything. And if two propositions coincide in import, we may be tempted to infer that by removing their subject we get two predicates that coincide in import. But this is just a fallacy. 'Smith shaves Smith' says the same as 'Smith is shaved by Smith'; but the predicates '— shaves Smith' and '— is shaved by Smith' are quite distinct in their import.

Secondly, it may be thought that (3) is an ambiguous predicate, whose import varies according to which logical subject we supply as antecedent to 'it'; in that case, (3) as occurring in (1) might after all coincide in import with (4). This impression can be removed if we consider a paraphrase of (3) from which the pronoun 'it' has disappeared, e.g.:

(5) — (is an epithet that) at once (unambiguously) stands for and lacks some property or other.

It is most important to see that (3) is *not* ambiguous in the way just alleged. If (3) had such ambiguity, then we might indeed suppose that ' the property for which it stands ' occurs in (3) as a referring expression that calls for a namely-rider; it would refer to now one, now another, property, according to the antecedent we supplied for 'it'. But if (3) is not thus ambiguous, we cannot ascribe a shifting reference to this phrase; and we clearly cannot ascribe to the phrase a *constant* reference either, for then it would have to refer to the same property in all occurrences of (3), e.g. in:

(6) ' German ' lacks the property for which it stands,
and (7) ' obscene ' lacks the property for which it stands.

We must therefore deny that ' the property for which it stands ' when it occurs as part of the predicate (3) *ever* refers to a specifiable property; the demand for a namely-rider is quite illegitimate.

The reader may find this argument pedantic. I have admitted that the last six words of (2), as opposed to those of (1), are an expression that calls for a namely-rider; well, would not Ryle's argument go over just as well if (2) rather than (1) had been the first line in his unpacking of the assertion that ' heterological ' is heterological? Let us see. The unpacking would begin with:

(2) ' heterological ' lacks the property for which ' heterological ' stands,

and the namely-rider that would be supplied is:

(8) namely, the property of lacking the property for which it stands.

Now, whereas in a way ' it ' in (1) was replaceable by ' heterological ' in quotation marks, no such replacement is possible for ' it ' in (8). (8) is tantamount to:

(9) namely, the property of being an epithet lacking the property for which it stands

in which the antecedent of ' it ' is not a mention of ' heterological ', but is ' an epithet '. In regard to (9), the question ' namely, which property? which property does it then stand for? ' is as silly as the Duck's enquiry given above; of course, different (heterological) epithets stand for *different* properties, not for some one property. If ' it ' in (8) were replaceable by mention of ' heterological ', as ' it ' in (1) was, then indeed the old demand for a namely-rider would arise all over again, and we should get the regress that Ryle supposes. In fact the regress never gets started.

A further example ought to show how tricky the pronoun ' it ' is, and how unreliable is the notion of a referring expression that calls for a namely-rider. In the proposition

(10) The only man who ever stole a book from Snead made a lot of money by selling it

the first ten words certainly seem to call for a namely-rider; and there would be no obvious incongruity in supplying one, say '(namely Robinson)'. But the first ten words of (10) can hardly be treated as a referring expression; for they cannot be replaced *salva veritate* by some other style of referring to the same man. From (10) and

(11) Robinson is the only man who ever stole a book from Snead

we certainly cannot infer

(12) Robinson made a lot of money by selling it.

For (12) is not as it stands an intelligible proposition at all. We cannot escape trouble by substituting for 'it' in (10) and (12) the phrase 'a book', which is the grammatical antecedent of 'it' in (10). The inference from (11) and the modified (10) to the modified (12) would indeed then raise no trouble; but the modified (10) has quite a different force from (10) as it stands—'it' in (10) is not a pronoun of laziness.

The logic of pronouns with antecedents was extensively studied by medieval logicians in their chapters *de suppositione relativorum*; the term '*relativa*' covered *all* pronouns with antecedents, like 'it' in this article, and not only *grammatically* relative pronouns. This medieval treatment of pronouns was inconclusive; and modern logicians have rather neglected the subject. The most important modern treatment is that of Quine, who has repeatedly pointed out the relation between pronouns of this sort and bound variables in logistic notation.[1] My aim in this note has been to show the need for a detailed and thorough discussion of *relativa*; and to deprecate the lazy assumption that such pronouns, or phrases containing them, can be disposed of by calling them 'referring expressions' and asking what they refer to.

University of Birmingham

[1] Cf. in particular the passages referred to s.v. 'pronoun' and 'cross-reference' in the index to his *Word and Object*.

THE HARDNESS OF THE LOGICAL 'MUST'

By EDWARD J. NELL

> 'But am I not compelled, then, to go the way I do in a chain of inferences?'—Compelled? After all I can presumably go as I choose! 'But if you want to remain in accord with the rules you *must* go this way.'—Not at all, I call *this* "accord".—'Then you have changed the meaning of the word "accord", or the meaning of the rule'—No;—who says what "change" and "remaining the same" mean here?. . . . 'But you surely can't suddenly make a different application of the law now!' . . . But if I simply reply: 'Different?—But this surely *isn't* different!'—what will you do? That is: somebody may reply like a rational person and yet not be playing our game.
>
> WITTGENSTEIN, *Remarks on the Foundations of Mathematics*, I, 113, 115.

1. The important thing to understand here is that Wittgenstein is not simply saying that different people might interpret the same rule differently. Nobody would quarrel with this, nor would it be very interesting. Nor is he saying that when there is a difference of interpretation, there is no final, ultimate, absolute way of saying who is correct, that the decision of any court may be appealed against. For the question at issue is not one of interpretation; it is much more radical. When one proposes a new or different interpretation, or challenges that of another, one must justify oneself, one must present reasons for one's views; for such a proposal, such a challenge, represents a commitment, i.e. an "opportunity cost", and it would not be a "proposal" unless it were a commitment. That is, one must appeal to the history of the rule, to the precedents, to the other related rules, one must draw inferences and state a case. The question of "interpretation" is the question of when and how to apply the rule to specific cases; it is the question of "use" in the sense of *operations*, but not of "use" in the sense of *function*; "how" to use a rule is not the same as "why" to use a rule, but both are necessary to the meaning of the rule. The latter gives the purpose, the former the interpretation. In a game of football, the rule 'keep the ball out of the goal' would govern the behaviour of the goal-keeper. The interpretation of this rule, its application to specific instances, the answer to the question 'how?', would be in terms of specific types of kicks, blocks, taking up certain positions and stances at various times, etc.; but the purpose of the rule, i.e. the function of the goal-keeper, the answer to the question 'why?', would be in terms of the object of the team, and the structure of the game, what counts as 'winning'. It is only if he knows the answer to the latter that he can imaginatively, or creatively, interpret the rule; that is, he not only should prevent goals from being scored, he should "help his team to win". But this last only makes sense in terms of the game as a whole.

The point here is, that the question of interpretation only makes sense, can only arise, when people are agreed upon the question of purpose. Suppose two players, together constituting a team, are to co-operate together; one of them says, 'I call this "winning"', the other, 'But I call *this* "winning"'! How are they ever to separate the functions, work together as a team in co-operation? But, it may be argued, all they are disagreeing about is the "interpretation" of the rules that define 'winning'. However, this is impossible, for until they have agreed upon what constitutes winning, until they have agreed upon the object of the game, *there is nothing to interpret*. Only when there is agreement upon what they are co-operating towards, or competing for, does the game actually come into existence, in their behaviour. Or, to put it another way, either one accepts, and agrees upon—this is what is involved in "acceptance" here—the *constituent* rules of the game in such a manner that each player knows both that all players agree and that all players know they all agree; or one cannot play the game at all, for no game will be defined, nothing can count as a 'move'; hence there can be no quarrelling about the "interpretation" of various moves.

2. What Wittgenstein appears to be saying here is that someone might reject outright the *constituent* rules of the game of logic; that is, he would not merely propose different applications of, or restrictions on, the rules of inference as we know them, nor would he be quarrelling over the validity of specific inferences (all of which would involve stating reasons, giving proofs, hence playing the game), rather he would refuse altogether to accept our rules! 'I call *this* "inferring"!' That is, he would refuse to play the "language-game of truth-functions", at least in any form that we know it. But Wittgenstein suggests a good deal more, as well. First, he suggests that someone could do this, could reject the laws of inference, and still "reply like a rational person", still be able to communicate. Secondly, he suggests that someone might *decide* not to "play the game", might "refuse" e.g. to accept a conclusion validly derived from the conjunction of some set of premises. It is not a question of the person not understanding, not "seeing" how the derivation went; but that in full knowledge of this, he might yet, in some meaningful sense, reject it. Thirdly, he suggests that logical rules are the same as any other sort of "normative" rules, that the "language game" of logic is much like any other, although if one does not play it, one is liable to suffer *practical* consequences in one's social and business contacts. '. . . the laws of inference can be said to compel us; in the same sense, that is to say, as other laws in human society' (RFM, I, 116). '. . . If you draw different conclusions you do indeed get into conflict, e.g. with society . . .' (Ibid.)

3. To take the first point: Surely if someone really refused to, or

did not, see identity and difference in the " same " way as we did, a conversation such as that at the beginning of this paper (RFM I, 113, 115) could not take place, for such a person could not *recognize* disagreement. Not only could the person not " reply like a rational person ", he could not reply at all; for it is a necessary condition for the existence of a language that there be public concepts, systems of classification such that each knows that everyone both knows and knows that all others know the function of each of these systems, and its relation to others. For this to be the case, it must be possible to both detect and correct *mistakes*; likewise there must be a decision-procedure for quarrels over interpretations, over the specific application of these sets of concepts to particular circumstances. We may agree on the definition of X, and yet disagree violently over what is to count as an instance of X, e.g. ' murder '; that is, we may agree on what is *essential* to X, i.e. what is both necessary *and sufficient* in the absence of any of the members of some list of " extenuating circumstances ", which in many cases it will not be possible to list exhaustively. Hence interpretative disagreement will always remain a possibility. This is less common in the world of natural phenomena than in that of social.

But to detect a " mistake " is precisely to say, ' that is not the same as this, and this is correct '. Hence to recognize mistakes in following rules, one must be able to recognize ' the same ' and ' different ' in the same way as other people. Accordingly, if one says, ' But I call *that* " correct " ', one may reply, with perfect justification, ' So what? Nobody else does, *and you know it*.' Consider: if a conversation is to take place, there must be communication, and for this it is at least necessary that both know that the other knows what each is designating, referring to, e.g. by means of demonstratives. Thus, the same person who asks, ' who says what " change " and " remaining the same " mean here? ', must himself know the answer, in order that ' here ' be meaningful, and moreover, he must know that the person with whom he is arguing also knows; hence he must be conscious that he knows it, for if the other did not know it, he would have to behave differently, i.e. explain what he meant to the other. That is, the conversation could not take place unless ' here ' meant the same thing for both, and both knew it; hence in this instance both agree and know they agree, on what ' change ' and ' remaining the same ' mean, i.e. how to use them, and the function they serve. Likewise, when the person says, ' I call *this* " accord " ', or ' this surely *isn't* different ', he must give a sense to ' this ', he must designate a particular something, which is either differentiated in some way or ways from the rest of the universe, or is not. If it is not, then the " conversation " is meaningless. But if it is so differentiated, if it is a particular something, then ' this ' either designates the *same* something for both of them, about which they disagree as to whether or not it,

this thing to which both know both are referring, does or does not fall under a given rule (the *same* rule, i.e. identical to itself, for if different rules were being referred to, there would be no argument, or a different one); or it does not, i.e. ' this ' designates something different for each of them, in which case they have been talking at cross purposes, and there is either no argument or a different one. Suppose neither knew to what the other's ' this ' referred, what it designated; how then could they know whether or not they agreed or disagreed? Consequently, either no conversation is possible, or both parties must accept, and know that they both accept, identical meanings for ' change ' and ' remaining the same ' in this situation—though they may argue about the " interpretation " of these rules. That is, for the conversation above to have taken place, the second person, in making his reply, would have to accept, in the process of doing so, that which his reply itself was rejecting; and more important, perhaps, he would have to know himself that he was accepting, in making his reply, that which his reply was rejecting; for unless he knew this, he could not know what it was they were disagreeing over, or even if they were disagreeing. Hence, not only is he contradicting himself, but it is a necessary condition of this conversation that he knows he is doing so, i.e. it is an exercise in " double-think ".

4. The second point raised was that someone might decide not to, or refuse to, accept the rules of inference. That in a manner not essentially dissimilar from the way one might refuse to play chess, one might refuse to play logic. Wittgenstein points out that such a person will suffer severe consequences, much as he would if he broke the other laws of society—' the clerk who infers as in (17) *must* do it like that; he would be punished if he inferred differently ' (RFM I, 116). Presumably, ' punished ' does not necessarily mean institutional punishment, but rather that he would fail in his contacts with other people, he would be unable to achieve his goals, unable to co-operate effectively with others, and this is certainly true. But could one possibly, conceivably make such a decision? For in the process of making it, comparing alternatives, weighing the advantages and disadvantages, and in communicating one's refusal to accept the rules, one would necessarily be playing the game! And moreover, one would necessarily know that one was doing so! That is, it is always possible to contradict oneself, to " double-think ", but breaking the rules is not the same thing as not playing the game; indeed, it *entails* that one *is* playing. This leads to the third point: the rules of inference are fundamentally different from many (though not all) other normative rules, and laws, in that, while they may be broken, they *must* be accepted; one cannot *not* play the game of logic, and remain either human or alive. The mere attempt to fill one's basic physiological needs requires that one classify things with respect to them, that one

differentiate between what is oneself, and what is not, that one be able systematically and accurately to re-identify aspects of one's environment; to do this, one must be able to say what is the 'same' and what is 'different', and this is playing the game of logic, if only to oneself. With communication, the case is even more obvious; a contradiction, *as such*, communicates nothing, though it may very well be a *sign* of something (e.g. insanity, poor logical technique, that a mistake has been made) or even a *symbol*, e.g. 'it's both raining and not raining right now' may stand for a state of the weather not adequately accounted for in the normal systems of classification. But the violation of the rules of inference, considered in itself, as such, necessarily prevents communication. The point is, one cannot be oneself, one cannot be a member of society, one cannot achieve, or even define, one's goals, purposes, and intentions, let alone discover the appropriate means to these, unless one can draw correct inferences. Wittgenstein says of violating the rules of inference, 'there is even something in saying: he can't *think* it. One is trying to say: he can't fill it with personal content; he can't really *go along with it*. . . . It is like when one says: this sequence of notes makes no sense, I can't sing it with expression. I cannot respond to it. Or, what comes to the same thing here: I don't respond to it'. But the point is, he can *think* it, anyone can think a contradiction; and he can respond to it; he can even believe it—we see this only too often; what else is "having faith"?—but what he *cannot* do, once he understands the rules (mistakes apart), is call it a 'valid inference'. To the extent that he does, he will fail to communicate (i.e. fail to fulfil the necessary conditions for 'calling').

Nuffield College, Oxford

The Editor will be visiting Brown University, in the United States, between January 15th and June 15th, 1961. During his absence, contributions and other correspondence should be addressed to the Acting Editor, Mr. P. T. Geach, at the Department of Philosophy, The University, Birmingham, 15.

e able
ment;
hat is
neself.
ction,
ign of
been
now'
in the
infer-
ation.
ciety,
tions,
draw
ence,
ng to
ith it.
nse, I
nes to
e can
to it;
aving
takes
s, he
s for